Review of "A new global grid-based weighted mean temperature model considering vertical nonlinear variation" by Peng Sun et al.

General Comments:

The calculation of Tm is not a new issue. This work developed an empirical Tm model that takes the impact of altitude variation on Tm into account. Some interesting results were obtained. The manuscript is well written. However, I think some additional works are essential before considering the publication of this study.

Specific Comments:

We have seen from broader scientific publications that Tm was modeled by many authors by looking at the global or specific region or use the satellite techniques such as GNSS, GPS_RO, and climate reanalysis datasets. However, in my opinion, we should pay attention to some points

1. In the Introduction, you wrote that "not all GNSS stations are equipped with meteorological sensors." then you said the Tm models independent of meteorological observations "had to be constructed". Really? I don't think it is the only solution while there are some other methods to solve such a problem. For example, we can interpolate the measurements from nearby surface meteorological sensors to the GNSS stations followed by using the Ts-Tm model, or we can also interpolate the reanalysis vertical profiles over the sites. Right? So you should write more to convince me of the significance of your study.
2. What is the application area of your Tm model? For time-critical applications? Your Tm model is based on the ERA5 monthly mean reanalysis data. Theoretically, such monthly mean reanalysis data has no ability to capture the short-term variations of Tm. Furthermore, your Tm model is independent of real-time meteorological observations. Therefore, I am not sure about the ability of your Tm model for near-real-time applications. Maybe the error statistics of your Tm model is good. But these statistics indexes were also the "mean precision index" over a specific period. For near-real-time application, we should also pay attention to the short term performances of the Tm estimations, especially under some extreme weather conditions. I would like to see your discussions about these issues in detail. Giving some time series of Tm over some points may be helpful.
3. Or you can use your Tm model for climate research. Unfortunately, I didn't see any discussions about this. In fact, there are still some questions about climate application. What is the advantage of your model compared with other solutions, e.g. interpolation of reanalysis data? Are there enough GNSS observations located in "the ocean area, a high mountainous area, or even a flight vehicle" for demonstrating the advantages of your model in climate or weather issues?
4. I agree that Tm is "a crucial variable for the determination of the conversion factor Π". However, the significance of Π in determining GNSS PWV depends. Equation (13) in your study is not quite accurate. It may greatly exaggerate the impact of Tm errors on PWV calculations in many cases. Detailed discussions about the uncertainty budgets of GNSS PWV can be found in https://doi.org/10.5194/amt-9-79-2016 or https://doi.org/10.5194/amt-12-1233-2019. We can see that under some situations the barometric pressure observations may introduce much larger errors into the GNSS PWV estimations. So your serious discussions about the

improvement in GNSS PWV calculations brought by your Tm model will be grateful.